Referee #2 responses

(i) The manuscript has incorrect analysis and discussions on some of the data. In particular, some of the 3-minute measurement data is misinterpreted. It should be noted that this data is not statistically converged.

(ii) The conclusion and abstract need to indicate more of the limitations of the presented measurements.

(iii) Line 26 states some limitations, but this needs to be mentioned more in the data discussion to explain certain observations.

(iv) See a list of incorrect/inconsistent analyses and statements in the manuscript.

The manuscript presents results from both experimental and numerical analyses of high-resolution temperature series inside the Pi Chamber, with the former consisting of temperature time series measurements of 3 and 19 minutes. The chamber's primary objective is to investigate microphysical processes relevant to cloud formation development and decay, such as temperature, humidity and supersaturation fluctuations. Characteristic time of such processes is of the order of seconds and tens of seconds. The referee's primary concern is that the 3-minute data are too short to achieve convergence, as reflected in the skewness distribution in Fig. 6. We acknowledge this limitation; however, 3 - minute series provide valuable information on small-scale variability and inhomogeneity within the chamber useful for the microphysical applications, while longer-term temperature properties useful for characterization of RBC inside the chamber are documented in the converged 19-minute series. The inclusion of the 3-minute data also serves to support our discussion on thermal plume dynamics and temperature variability relevant for atmospheric aerosol-cloud interaction experiments in the chamber. These short time series serve different purposes and complement the RBC analysis.

Concluding, we substantially modified the text of the manuscript, adding relevant information to the abstract and discussion sections addressing the constraints of the 3-minute data.

-- Figure 3: Unclear what is learned from this figure.

The picture shows a simple schematic of our setup to visualise the used measurements methodology. It supports the description in the text.

-- Line 28: Suggest you do measurements in the boundary layer, which is incorrect. Boundary layer thickness is well below 1 cm for Ra=1e9, so the sentence should be removed.

Our intention was to indicate that the statistics we obtained in the Pi Chamber are comparable to the previous studies we provide references to. Those measurements were conducted in the boundary layer of RBC. For clarity we have improved this sentence.

-- Line 52: Fan et al not relevant

We disagree with the referee. The whole section III in this article discusses problems of RBC and discreetly heated surface which have direct links to our study, especially in terms of thermal plumes and atmospheric boundary layer connections. The paper also provides a broader perspective on natural convection which can provide better understanding of the problem for the readers loosely related with the RBC studies.

-- Line 55: Olsthoorn, 2023 not relevant

Context of line 55 was to show diversity of RBC problems in the literature, here shown from the theoretical side. It is also another example of translation of RBC consideration to geophysical context.

-- Line 71: Brown and Ahlers's most famous model on LSC is not cited, and not all given references are relevant

It has been included now in the manuscript, thank you for the suggestion.

Since no additional references have been explicitly identified as irrelevant, and from our perspective citations we included are relevant we cannot address this remark.

-- Line 77: See Annu. Rev. Fluid Mech. 2010. 42:335–64

Thank you for the suggestion, the paper is more focused on the structure functions analysis, however, we have added the citation for a better context of our study.

-- Line 143: Deteremination --> Determination

We have improved this typo.

-- Line 155-156: Why are no differences observed near the top plate? Roughness should still affect the flow.

In line 120 we state that the flange at the top was not ideally insulated from the outside conditions, additional comment is provided in line 160. This might be the main reason the roughness influence could have been suppressed.

-- Line 172: "All these effects are beyond the scope of this investigation, but the raw measurements give clear evidence of changing oscillations near both plates." --> This statement is incorrect. The fluctuations shown in Figure 4 cannot be compared to the LSC measured as performed in the RB community.

We have improved the sentence so that it does not confuse the reader. The meaning behind this part was to outline that temperature time series reveal some kind of irregular oscillations which might be attributed to LSC effects or other overlapping dynamic processes.

-- Scatter in these figures indicates that longer measurement data would be required. Performing measurements longer than 19 minutes would reduce fluctuations in statistical data.

We kindly draw the referee's attention that the main goal of the presented research is to explore small-scale variability of the scalar field which is of importance for microphysical processes in clouds, e.g. in terms of supersaturation fluctuations. The part on this starts in line 21 where we also included some citations of the recent works in a turbulent environment provided with the Pi Chamber facility.

-- Figure A1 and Figure A2: Temporal comparison between experiments and simulations is misleading as no one-to-one comparison of the same flow state is performed.

The intention here was to show two realizations (for each plate) indicating maximal fluctuations rather than one-to-one comparison. If we consider processes in the real atmosphere or in laboratory conditions, each particle in the flow experiences only local field fluctuations. These two overlapping curves exhibit quite similar magnitude of fluctuations between the DNS and our experiment despite the fact these two had different time resolutions. We have added an additional sentence to Appendix A to highlight the intention behind these plots.

-- line 195 and further: Discussion incorrect as the data are not converged. This data is not converged, and that discussion is meaningless. The 19-minute-long measurements do not show such an increase.

We agree with the referee that longer time series do not show the increase in skewness which is present in shorter records. It is put explicitly in line 193. However, we disagree with the statement that the following discussion is meaningless since it explores the potential reasons why we observe such change in skewness distribution near the lower plate. The timescale of thermal structures evolution is of seconds order which are averaged out in longer measurements. This is why even though 3 min data are not converged in the bulk region, we still can observe evidences of more organized behavior near the bottom. Moreover, the obtained results are also supported by DNS in Fig. 11 showing very similar skewness distribution.

-- line 211: Indeed 3, minute measurements are too short.

We have answered this point at the beginning of the document.

-- Figure 7: Vertical variability of the LSC period with respect to --> LSC is the coherent large scale flow structure in the cell, so it should not vertically vary. The discussion should be adjusted accordingly.

We agree with the referee and have improved the caption.

-- Provide details on how Pearson correlation coefficients are calculated.

The formula we used the Pearson correlation coefficient *p* is the following, $p(A, B) = \frac{1}{N-1}\sum_{i=1}^{N} = \left(\frac{A_i - \mu_A}{\sigma_A}\right)\left(\frac{B_i - \mu_B}{\sigma_B}\right)$, where e.g. for Fig. 8 coefficients *A* and *B* are $\log\left(P(f)/p\left(f_p\right)\right)$ and $\log\left(f/f_p\right)$, μ, σ, and *N* are mean, standard deviation, and the number of observations respectively. For the best fit we also averaged the raw spectra over equidistant logarithmic frequency bins. The procedure is described in line 242.

-- Line 252: "relatively small variability of PSD slopes." --> The variations in the slopes are very large. Scaling exponents vary by as much as 20 to 30%. This severely limits how much one can learn from this data. That is recognized in line 254 ("Our analysis provides no clear answer,")

We have improved line 252. However, the slopes variation in the inertial regime increases as temperature difference decreases i.e. buoyant forces weaken. For

$\Delta T = 20$ K we observe slopes that exhibit smaller oscillations around -7/5 which then change near the top.

-- line 257-291: What main point do you want to explain to the reader?

In this part we investigate literature references to scalar slopes that would correspond to the results we obtained. This is why we explore potential connections with the other phenomena characteristics for 2D and 3D turbulence.
In the revised version we improved this paragraph and showed another link to thermal structures influence on scalar spectra resulting in the -3 regime which is based on a series of works by Chen and Bhaganaga (2021, 2023, 2024).
A comprehensive discussion of PSD scalar scaling in dissipative regime is provided by Gotoh and Yeung (2012) and Sreenivasan (2019) where it is indicated that for the Prandtl number of approximately 1 we do not know yet what the spectra in the roll-off region is. Some recent results for the energy spectrum have been provided by Khurshid et al. (2018) and Buaria and Sreenivasan (2020) based on direct interaction approximation by Kraichnan (1959), suggesting that this regime could be represented by an exponential form. However, here we show that power law might be sufficient for the scalar spectra in a buoyant environment.

-- line 339: It should be discussed that both measurement times are too short, especially the 3-minute data, which even leads to misleading discussions in the manuscript.

We have answered this point at the beginning of the document.

-- line 341: How this improves our understanding of atmospheric flow is not discussed.

This part has been improved. The meaning behind this was to point out that RBC experiments on thermal structures, like thermal plumes, might be very valuable in terms of better understanding of the nature of surface-air temperature fluctuations through statistical analyses.

-- line 368: Scheel et al. is a numerical study with no theory on heat transport.

We have improved the citation.

-- Figure 11: Why is DNS not symmetric around the mid-plane?

The plot represents single-column data (not the horizontal average) this is why the perfect symmetry in mean *w* and *T* skewness is not expected, in particular for the period of LSC circulation. The appropriate note has been added in the figure description.

-- Appendix B: "and display greater symmetry compared" --> Incorrect. Normalising the temperature difference between the plates does not affect symmetry around the midplane.

We agree and have improved this part.

-- Figure C1: What does it mean that velocity in the centre is always positive? Is this a velocity magnitude?

Yes, this is the magnitude of the mean velocity profile. The caption has been updated.

-- Figure C2: What are all the numbers in the legend?

In C2a numbers describe the positions of the UFT with respect to the chamber's height. In C2b these are the Pearson coefficients. Both graphs are a direct analogy of Fig. 8 but in wavenumber space.

-- Figure D1: The data are visibly deviating from the presented lines. Difficult to see how this corresponds to Pearson correlation coefficients of -1.

The Pearson coefficients are calculated for all three sets of data (3 min, 19 min, and the DNS) but the lines are fitted only for 19 min datasets (squares). We have added the missing information on this in the manuscript.